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On the Perturbations of Uranus and the Mass of Neptune.

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In the determination of the mean distance of the satellite of *Neptune*, and the consequent evaluation of that planet's mass, the faintness of the satellite has proved a great difficulty; so that different observers have differed systematically *inter se*, although each one is quite consistent with himself.

For instance, the mass of *Neptune*, from O. Struve's observations of the satellite, comes out $M = \frac{1}{14491} \pm 0{\cdot}02431\ M$, while Prof. G. P. Bond has deduced $M = \frac{1}{19400}$ from his father's and his own observations.

The perturbations of *Uranus*, by means of which *Neptune* was originally discovered, are, of course, adequate to determine its mass; and it would naturally be expected that the probable error of such a result would be much less than the difference between the numbers cited above.

Prof. Peirce, employing, I believe, the same observations as Leverrier did in his "Recherches sur les Mouvements de la Planète *Herschel* (dite *Uranus*)," has deduced (*Ast. Nach.*, No. 637) the result that, in general, a mass of *Neptune* nearly $\frac{1}{20000}$ will represent the motions of *Uranus* considerably nearer than a value more closely approaching to Struve's.

I was led to take up this question partly because it had been, some years ago, made the subject of a prize question by the Haarlem Society of Sciences (which, so far as I know, was not answered within the specified time), and partly because Struve's mass of *Neptune* seems to have been adopted by some high authorities; by Leverrier as, at least, a provisional value.

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The perturbations of *Uranus* produced by *Neptune*, assuming $\frac{1}{19400}$ as the disturbing mass, were computed by the method of mechanical quadratures. These were then added to the perturbations of heliocentric longitude derived from Leverrier's theory, and the theory thus computed (so far as heliocentric longitude is concerned) was compared, for one date in each year, with the results from Bouvard's Tables contained in the *Greenwich Planetary Reductions*, and with the *Nautical Almanac* since 1836 inclusive, to 1858.

Errors of these latter theories in heliocentric longitude were obtained from the *Greenwich Planetary Reductions*, and the *Results of the Greenwich Observations*, 1836–1858; and thus the errors in heliocentric longitude of the present provisional theory were obtained.

It is proper to mention here, that Bradley's observation of 1753 was omitted here, as it seemed desirable to found the result on modern observations, using the ancient ones as a confirmation; also that where the error of radius vector in Bouvard's Tables was found sensible, observations in the neighbourhood of opposition were used.

The Paris observations (1801 to 1819), the Greenwich (1831 to 1835), and the Konigsberg observations (1814 to 1846), were also used; the first series, as reduced by Leverrier, in the *Annales de l'Observatoire Impériale*, and the remainder after Flemming's (*Ast. Nach.* No. 755 *et seq.*), and Applebaum's (*Königsberg Observations*, vol. xxxii. p. 210 *et seq*) reductions. It is known that the observations at these Observatories were made chiefly near opposition; and, indeed, Flemming has computed (with a few mistakes) the opposition as it is derived from observations made near it for a good many years.

Of course, every effort has been made to put all these reductions in the state in which they would have been if uniform star-places and other elements had been used in making them.

The results were found to indicate with some degree of probability, by a mere inspection, that the disturbing mass would need but little change. In fact, a preliminary solution of a set of twenty-six equations obtained by equating the residual errors of this first theory (taking the means of three years together) to functions of unknown variations of the elements of *Uranus* alone, represented these equating with final remainders of less than 4″ in every case.

It seemed expedient, however, to introduce a variation of *Neptune's* mass as an unknown quantity; and after another preliminary solution, whose object was to obtain means for combining (as before) the observations of three years together, the weights were assigned to different series of observation, according to a somewhat arbitrary distribution. A final solu-

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tion then gave the mass of *Neptune* $M = \frac{1}{20039}$ with a probable error in the denominator ± 295.

The increase of *Neptune's* mass to $\frac{1}{14491}$ would have the effect of nearly quadrupling the mean error of observation; and would, in fact, leave intolerable discrepancies in several places.

An investigation now made into the geocentric longitudes derived from observations made before *Uranus* was actually recognised as a planet, gave eminently satisfactory results. The following is the comparison, with the observations used by Leverrier, p. 138 of his *Recherches*, "to form the first twelve equations, which are all ancient:"—

Year.	No. Obs.	Observer.	C — O.
			″
1690	1	Flamsteed	− 5·0
1712	1	,,	+ 18·1
1715	2	,,	+ 5·9
,,	1	,,	+ 0·8
1750	1	Lemonnier	+ 4·0
,,	1	,,	− 1·6
1753	1	Bradley	+ 0·2
1756	1	Mayer	+ 0·9
1764	1	Lemonnier	− 0·4
1768	2	,,	− 2·3
1769	6	,,	− 5·3
1771	1	,,	+ 5·0

Flamsteed's observation made in 1712 is uncertain, on account of the rate of the clock. It is isolated about 2½ hours from the comparison stars; and, during an interval of twenty or more days, for which there are only one night's observations, the clock was often very irregular.

Grouping the observations, as Leverrier has done (p. 239 of the Memoir above cited), we shall have as residual errors,—

	Error.	Leverrier's Limit.
	″	″
1690	− 5	25
1712–15	+ 8	15
1750	+ 1·2	15
1753–56	+ 0·55	10
1768–69	− 4·55	10

All these observations are, therefore, represented much within the limits given by Leverrier, p. 240 of his *Recherches*.